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MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-On-Hudson, N. Y.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

SPECIALIZATION AND RESEARCH IN THE MEDICAL SCIENCES[1]

MODERN scientists are not encouraged and are become less inclined, except in the afterglow of an active life, to indulge in metaphysics. The visualization of material phenomena, particularly when set in motion by deliberate experiment and observed in their successive stages, tends to replace speculation as to a more complete, though less verifiable series of facts. This reliance in the natural sciences on observation and experiment rather than on ratiocination is responsible for the great and rapidly increasing body of useful knowledge we possess.

Philosophical treatises by even conspicuous representatives of the natural sciences have seemed to me to differ from those of the metaphysicians in that the former apparently fail to appreciate that the metaphysical game is played subject to certain rules which have the same purpose of order as the rules in other games. Philosophy is apparently a subject like fine arts, about which many people think they have intuitional knowledge. We judge pictures as bad or good not on the basis of certain criteria that have come through the ages to be recognized as essential, but in accordance with whether we like or dislike them. In the same way we may think, because we have a certain facility in the exposition of scientific data, that we can offhand write

[1] Address of the vice-president and chairman of Section K, American Association for the Advancement of Science, New York meeting, December 29, 1916.

an explanation of the larger relations in as direct and convincing a manner as William James did his "Pragmatism." As example of attempts of this sort, I am thinking of books like Haeckel's "Riddle of the Universe," and Shaler's "The Individual." Metchnikoff in his "Studies in Human Nature" would seem an exceptional biologist who has taken the pains to learn the metaphysical rules, and it is an interesting proof of the modern discouragement of speculation on the part of scientists to be credibly informed that the publication of this book sufficed to debar its author from election to honorary membership in one of our most exclusive national societies. This discouragement reflects what I believe to be a fundamentally incorrect attitude in many of us toward metaphysics. We regard it largely through ignorance of its methods, and lack of appreciation of its heuristic value, as a grab bag into which are dumped all conceptions that can not be demonstrated, or as a method adopted from unworthy motives by the scientifically inaccurate.

So much in explanation of an attempt as an alien to speak a language with which I am not familiar, the expressiveness of which, however, I venture to think I appreciate. So much in extenuation of an attempted exposition of one phase of scientific method. The thesis of my remarks is that the best method of accomplishment in the medical sciences is to adopt the bloodhound method of nose to trail, to encourage ourselves in specialization and still more specialization, to dig deep rather than to spread smooth.

My traveling acquaintance, a lawyer, could not understand, when we passed the power dam skilfully blocking the mountain torrent, why I could not explain to him the essential principle of converting the energy of the foaming water into electrical voltage. "I thought you were a scientist," he remarked scornfully, "That is a scientific problem, isn't it?" I had no crushing retort ready for him, but I hope I may at a later day explain and perhaps justify my more or less deliberate ignorance to you, a more discerning audience. The public expect results, but usually misunderstand methods of obtaining them; they are willing to accept the greater returns following greater specialization, but do not always realize that effective specialization takes even more time than generalization, and to some extent excludes it.

I imagine that many of us, if we were to present an ideal system of intellectual self-development in graphic form, would sketch a pyramid with a broad base of knowledge representing the lower educational years, sloping and narrowing upward toward the increasing specialization of a life work. It is inevitable that each additional unit of knowledge, each brick in the structure we are raising, will eventually take its place in some definite relation to every previous brick in the mental edifice by which we represent to ourselves the external world. But is this ultimate structure the one we should have in mind in training ourselves as brickmakers? Do we not confuse this edifice, toward which we may contribute a unit, with the plan by which we develop as contributors? The pyramid is a not unpleasing and certainly an enduring structure; it met admirably the needs of a tomb for Egyptian kings; it may serve as a dignified mausoleum for acquired facts, but the more rapid acquisition or reception of new facts may be better served by an essentially different construction. Certain more modern needs are better met, according to Signor Marconi, by a very thin and lofty antenna. May it not be that the wireless outfit resting on no considerable base, though carefully supported by connecting

strands, typifies the modern method of development of one's powers for productive scientific work? Does not this delicate apparatus, shooting up straight from the earth, allow expansion into the unknown which the self-limiting convergence of the planes of the pyramid excludes?

At all events, it is no longer possible for one to master all, or even several contributory sciences, before turning his attention in a productive way to one of them; there is not time or strength enough. We are no longer in the middle ages, where a genius like Dante could reflect all knowledge that had preceded him in a set of scholastic and poetical treatises, or another like Leonardo da Vinci could contribute to several arts and sciences methods that were fundamental. I appreciate, I believe, the surprising vigor of Leonardo's intellect, but am not willing to admit that his astonishingly successful versatility proves him a type of superman that has ceased to exist. I feel sure that Leonardo's intellectual equal may well be among us to-day, but could never by any chance make notable contributions to subjects so diverse as painting, sculpture, engineering and mathematics. This would seem to prove not that the race of man has fallen off, but that each of the subjects has so grown in complexity as to require a lifetime to master. It is no little factor in success in any subject to be early in the field, to be the first explorer. In many respects it requires greater powers of observation to detect further important details in a landscape, the important and perhaps more obvious features of which have already been described by another. The earlier observer, moreover, has the undoubted advantage of entering on his work with a mind untrammeled by the notions of numerous predecessors.

The most modern equipment for scientific advance need be burdened with no very heavy impedimenta of fact—the newer science develops or rediscovers the methods of other sciences at need without having mastered their content in fact. To justify this light-marching order, which I venture to recommend for the scientist in his invasion of the unknown, I must outline my conception of the nature of scientific progress and then discuss to what extent each science is dependent on other sciences in this advance. Let me repeat that I have in mind primarily the newer biological sciences, particularly those that relate to medicine, and am considering them in relation to one another and to the more fundamental sciences of mathematics, physics and chemistry. My remarks doubtless do not now apply to these latter fundamental groups which seemed to have developed into a more closely correlated and perfect whole where interdependence seems more constant. Am I not correct in assuming that in its early development, chemistry, for example, was less dependent on mathematics and physics than it is to-day? May we not look upon these three sciences as similar in their growth to three adjacent trees which at first stood clear from each other, but which in their further development have intertwined their branches and roots so that they now appear from a distance as more nearly an entity?

At all events progress in the biological sciences depends, first, on discovery of new facts by purely observational and by experimental methods, and, secondly, on the elaboration of hypotheses and theories as a means of uniting these data and as introductory to more facts. Let us consider in some detail the method by which each of these advances is made and in what respect knowledge of kindred sciences is essential in this analysis and synthesis.

It seems obvious to us now that proper appreciation of any scientific phenomenon

must depend first on a knowledge of its component parts and their functions. This analysis or dissection not only must precede, but seems at once more intimately scientific than the synthetic stage that follows. I here use the word "scientific" in the specialized sense of acquiring data concerning natural phenomena. The second or synthetic stage is more metaphysical in that it considers data that have been acquired in their relation to one another. The first phase is more intimately scientific, then, in that we are actually in contact with those elements which we describe as facts. The second or synthetic stage is, however, fully as essential to progress in that *without* it we should never pass from a known group of facts to one that is unknown. The synthesis of small or less certain groups of facts gives rise to the working hypothesis which, in its proving or disproving, leads to other facts. Larger or more certain groupings of fact constitute a theory which, in its restatement of evidence, serves as a point of departure for further advance. A theory may stand for an indefinite period as a complete statement of the facts with which it deals, or it may soon be supplanted by a better one. In either case it has its heuristic value.

In eliciting facts certain methods are required, in the larger sense methods of discovery, in a more restricted sense methods of technical precision. It might be thought that in methods of discovery, certainly, a knowledge of the methods of other sciences would be essential, and so indeed they are, but in no exclusive sense. It has never ceased to surprise me to find from conversations with my colleagues in other branches that all experienced investigators employ the *same* methods of discovery—the materials we handle may be as diverse as you like, the technical details incomprehensible to one another, and yet the methods of attack on the unknown remain the same. We all gravitate through experience into the same channels of reasoning, the same methods of planning experiments, of erecting working hypotheses, of rejecting them when they fail of verification, or of trying them further when they pass the first test satisfactorily.

There remain, then, methods of technical precision. For the purpose of this discussion of the usefulness to the biological medical sciences of more fundamental or of merely contributory sciences, we may consider methods of technical precision as statistical, instrumental or experimental. No claim is made as to the inclusiveness of this cataloguing.

There is some dispute, I believe, as to whether statistics constitute a separate science or merely a method. At all events, statistics are used as a method in all sciences or groupings of fact. Of late, statistics are used to a large extent in certain biological work, notably in the branch of hygiene dealing with vital statistics, and in certain more theoretical branches as the laws of heredity (Mendelism). It is obvious that any science which in its analytic phase accumulates a mass of figures or data will need statistical methods. I am not aware that statistical methods can be learned apart from the constituent facts which they are aimed to elucidate. It seems to me that such methods are best learned by using them, and that there is no particular object in learning the use of statistical methods in reference to wages, let us say, for the purpose of applying them in investigations of the incidence of tuberculosis. In either case we must refer to treatises written by those who have used statistics extensively both for the general methods and causes of error involved in their use. Statistics, to repeat, is not a separate science, but a

method employed at need, and a part of any science that uses them.

We have next the use of methods of precision. This may imply the use of a piece of apparatus, or a reaction that has been of service in another science. The use of such a piece of apparatus may suggest itself synonymously with the needs which it was intended to meet. Thus, if in one of our biological products we have reason to wish to measure total nitrogen or amino nitrogen, we should undoubtedly turn to a chemist who would suggest the Kjeldahl or the Van Slyke methods. The reference suggests at once what I should regard as the best method of reapplying the methods of one science to another science, namely, collaboration, or intimate contact with specialists in various branches. The man who thinks he is trained in one science by having passed through it a few years before, may well fall into the error of using methods he has learned rather than better methods since discovered and currently employed by specialists. A personal example may illustrate this fact. A few years ago one of my associates and I were working on a problem which finally required a chemical estimation of the amount of glycogen in the liver. This determination necessitates the rapid reduction of glycogen to glucose, followed by its quantitative estimation from the amount of copper oxide reduced. Fehling's technique had been the classical method followed in such estimations. Not trusting to our own judgment as to superiority of this method, we consulted a graduate student in the department of biochemistry who was working constantly with glucose determinations of this sort, and, following his advice, adopted the modification of Fehling's method which had recently been made by Bertrand. A few months later, on visiting a large eastern hospital where determinations of the amount of glucose in the blood were being carried out, I learned that six months' data had just been discarded, owing to the fact that the physician who was conducting the experiments had trusted his rather unusual training in biochemistry and had overlooked Bertrand's important modification, which he later adopted and which we employed throughout our study, owing to the fact that we had deferred to the opinion of a specialist.

It is doubtful if methods of experimentation, purely speaking, can be carried over from one science to another. We have stated that the methods of discovery in the broader sense are the same in all sciences, however different the component factors may be. In methods of experimentation, however, variation in factors counts. I have constantly been struck with the fact that the chemist experiments in a manner that is essentially different from the one which my work demands. Chemistry is a far more exact science than experimental pathology in the sense that the factors with which chemistry deals are better known. It is interesting to note, however, that a chemist may, and frequently does, accept certain biological evidence as proved which we should reject as inconclusive, owing to the omission of certain controls or checks. This difference in viewpoint is dependent on the failure of the chemist to appreciate certain fluctuations in living material which it is impossible now and will perhaps to some extent ever remain impossible to determine at a given moment. It does not suffice, moreover, to determine the mean of such a variation in a great number of instances, for the purpose of obviating controls in a given experiment.

In dealing with the interactions of two substances in chemistry we have to begin with, under the simpler and usual condi-

tions, union in fixed and in multiple proportions. It is true that in reactions between a weak acid and a weak base there is union in variable proportions, so that a series of compounds are formed. But in general it may be said that in chemical reactions the results may be foretold when the effect of controllable factors such as dosage, temperature, atmospheric pressure and the like, have been determined. The substance concerned in the reaction, and the conditions that affect it, have been rigorously tested and are understood, so that a given result can always be counted on. The experiment controls itself when properly performed. On the other hand, no one can tell what will happen if he injects a million staphylococci into the ear vein of a rabbit. The animal may be dead the next day with no evident lesions; it may die a week later with abscesses in various parts of the body, or it may show no symptoms and recover perfectly. These disparate results are due to the fact that in an experiment of this sort we are confronted with two sets of variable conditions inherent on the one hand in the living microorganism that is injected and on the other in the experimental animal. We recognize the existence and to some extent the range of certain of these variables, but remain ignorant of many of them; the majority of them are inherent in the condition we designate as life and disappear in death. It is incorrect to assert that our ignorance of them is due to an interest in vitalism. We are free to admit that our science is very young, that our data are relatively few, and that our ignorance of the factors concerned is great. And yet we have a group of significant, reliable and practical phenomena that we can reproduce at will when we handle these variable factors in our own way. Many of our reactions, although indefinite from the standpoint of chemistry, are of a delicacy that chemistry rarely, if ever, attains. The point of interest here is that the experimental methods of present-day chemistry not only have not led us to new facts in our field, but do not help us much to explain or control our present ones. In the experiment cited we can not assert from previous experience exactly with what point in the range of either variable factor we are confronted, we can not previously determine our conditions and know that they now actually exist. We know in a general way that in the experiment I have outlined we have to deal particularly with fluctuations in the virulence or pathogenicity of the staphylococcus concerned and with variations in the resistance to infection in the individual rabbit. Our type of experiment, then, is never complete unless we introduce numerous simultaneous and external controls. In the particular problem I have cited, we find that although one million staphylococci killed Rabbit No. 1 yesterday, a subsequent transplantation of the microorganism fails, in the same dose, to kill Rabbit No. 2 to-day. It could be determined that this result is due to a loss of virulence in the microorganism by the introduction of Rabbit No. 3 which is given twice the dose and dies as did No. 1 yesterday. Individual variations in resistance may, to a great extent, be avoided by choosing for the experiment rabbits of the same weight, raised under the same conditions, or, better still, from the same litter.

As a further illustration of the difference in viewpoint between the chemist and ourselves, let me suggest that the tendency of the former on entering our field of activity would be to devise a more precise method of estimating the number of bacteria used in the experiment rather than to introduce such controls as I have mentioned.

The problem I have given you is one of the simplest with which we have to deal. Conceive of the far greater complexity if we introduce an immune serum against the staphylococcus in such an experiment designed to increase the resistance of the rabbit to which it is given, and you will imagine where the real complexity of our science begins. Such a serum differs in its potency with the individual animal that has produced it, with its age after withdrawal from the animal body, and with the method by which it has been conserved; in other words, it introduces another variable factor. I may again define our mode of experimentation as differing from that of chemistry in requiring the introduction of simultaneous, external controls, the object of such controls being simply to define the effect of those conditions which we recognize as contributing to a given result.

Such differences as these, then, lead me to think that even great experience in one type of experiment will not fit one directly for experimentation of another sort. I do not mean to intimate that training in methods of precision is not of value, however different the conditions may be, but the best training for a given end lies in work and more work with the intrinsic materials involved, not so much as leading to greater technical accuracy as tending towards the establishment of an essentially specialized experimental viewpoint.

We come now to mention the value of multiple scientific experiences as fitting one for the larger synthesis or generalization in a given science. I have not reached that age where such generalizations as I mean appeal to me as the more important field in the experimental sciences, although I recognize that they are eventually necessary to present our work as a whole and in its practical aspects to the world at large. Such generalizations do, of course, imply factual knowledge of the wider sort, and I must confess to being awed at times by the aptness of apparent analogies between the better-known conditions which exist in one science in explaining formative theories in another science. Personally, I also usually doubt the rigorous exactness of the conclusions drawn in respect to the significance of any one science by one who handles freely the data of several sciences. I suspect at once the reportorial viewpoint, the existence of second or third hand, and ever so slightly garbled information. I am inclined to trust the solution of my problems to a combination of specialists rather than to the superman. Here again I plead for collaboration.

In our great, vital and complex science of medicine we can see, I think, an illustration of the ultimate value of intensive specialization and of deliberate or chance collaboration. Out of indefinite, speculative, empirical, bedside methods of the practitioner, have emerged, through the stimulus of the exact sciences, a growing number of increasingly accurate and effective laboratory branches. These laboratory sciences have become of practical value in the diagnosis, prevention and cure of disease, precisely as they have become separate entities and have fallen into the hands of whole-souled and intensive specialists. I make no mention here of the intellectually satisfying value of a concrete body of similar facts which constitutes a science. The relatively rapid applicability of the data of laboratory medicine to human welfare is at once an enormous stimulus to accomplishment and also a potential danger, owing to the possibility of too rapid generalization and application to meet a practical need. There are many who are impatiently waiting with individual needs in mind to apply any method of apparent value we may devise, and it

requires at times no little self-restraint to withhold an apparent innovation for greater certainty. Over-enthusiasm greets the advent of every fact that has the least suggestion of practical value. We have ourselves lived through successive eras in medical progress when from each group of specialists was expected the last unraveling of the human mystery. Morphologist, physiologist, bacteriologist, and biochemist has each had his turn. The ultimate truth lies in all these sciences, and again in no one of them alone. The danger to sober advance is not in the successive enthusiasms with which each specialty has been received, but in the dabbling methods of a group of investigators who have attempted to "follow the ball"; investigating a given medical problem in successive years by the latest method in vogue, becoming rapidly in turn pathologist, physiologist, chemist.

The ultimate solution of each medical problem lies in the combined attack of a group of investigators converging from different points of the scientific compass, each trained in a separate method and employing it intensively. The problem of cancer, for example, is now being studied by the morphologist who describes hitherto undifferentiated structures in the malignant cell by special staining methods; by the immunologist who demonstrates the presence of reaction bodies in the serum of cancerous animals and human beings; by the chemist who shows that certain substances given parenterally inhibit or stimulate cell growth, or who produces similar results by the use of various diets; and by the expert in vital statistics who shows the actual increase or decrease in incidence of the disease; by the biologist who shows in Mendelian tables the heredity of the disease in animals; or, again, the effect of cross-breeding on transmission of the tumor; and by the physicist who demonstrates the effect on the tumor growth of X-rays or radium. I have not exhausted the category, but merely wish to indicate that the significant advances in each of these methods of approach are made by specialists. Do not misunderstand me to mean that any one of these investigators may not be led by his work to assume seriously and purposefully the activities of any other type. Pasteur was a chemist who became a biologist and probably the greatest contributor to medicine, although without medical training, because he followed his problem to the bitter end into whatever field it led, with little regard for the fact that he was, technically speaking, unfit to encroach on medical territory. He rediscovered medicine from a new angle, untrammeled by any preconceived notions of how disease was regarded. Ignorance of veterinary medicine did not prevent him from isolating the causative agent of anthrax in cattle and from utilizing an attenuated virus in its prevention. Failure to have studied the central nervous system of man was no obstacle to the man who discovered the essential cause of hydrophobia and the means of preventing it. Imagine insisting that Pasteur's curriculum should have included medicine as a necessary prerequisite to the discovery of the fundamental principles of the infectious diseases.

I hope you will not take my remarks as indicating anything but the highest appreciation of instruction in the sciences in general as the best training for the youthful mind, or as contributive to general culture. You will not accuse me of advocating early vocational training without a preliminary survey of the realm of knowledge. To be specific, you will not imagine that I discredit the now universal requirements that premedical students should acquire a modicum of chemistry, of physics, and of

biology as furnishing an intelligent, scientific viewpoint for their subsequent study of medicine. Such a survey is not only good, but very properly prescribed as necessary. My remarks have been directed at a very different level and type of intellectual development from this; we have been considering our own particular problems as investigators. What I have been interested in discrediting is the persistence of ideas of machine-made education into the productive years of scientific life; the idea that if we seek eventually to become effective we should take care to perfect ourselves laboriously in each of the branches that have been regarded as fundamental. There is a real danger that we may spend our lives preparing ourselves for an indefinite piece of work that we never even start. It is, of course, much easier to continue preparing ourselves, to keep our scientific judgment strictly symmetrical by endeavoring to fit in each contribution that *others* make into its proper place, rather than to insist that one particular piece of work must be done *now* and to the exclusion of everything else. This insistence, however, I consider to be the true raison d'être of specialization, the only basis of real productivity.

These remarks, to repeat, are not a recommendation for educational anarchy, but an explanation of how a somewhat one-sided development may not only not be inconsistent with, but indeed the very essence of highest accomplishment. This is not so much a recommended program as an explanation of how things really work out. It is intended to some extent as helping to dispel the discouragement that I believe has come to many of us when we cease to be mere recipients of information and in a position to think and to do for ourselves in a chosen profession. I must confess to many hours of doubt for more years than I care to admit, as to whether I should really accomplish *anything*, owing to the fact that I had failed to become a good chemist *en passant*. It was always and increasingly too late to turn back and repair the errors or omissions of education, and as my problems finally gripped me instead of merely inviting me, I silently gave up the struggle to remodel my life. And in following some of these problems in certain of their ramifications, I found that although I could never hope to learn chemistry, I was curiously enough collaborating in investigations that utilized that very type of chemistry which my work required. I was absorbing in this intimate way certain very restricted forms of chemistry in the making.

Out of such experience has gradually formed a certain working philosophy, or, better, a philosophy of work which I have tried here to present to you. Those of you with less limitations may well question much that I have said, you may assert that breadth does not of necessity mean superficiality, and per contra, that digging a hole does not necessarily mean that it is deep, but in certain respects I am sure you will agree with me. Specialization in science, even in the narrowest sense, is essential to real accomplishment. Any extension of knowledge is dependent on an attentive consideration of a relatively small group of facts to the temporary exclusion of less related facts. To a great extent the smaller the group the greater the concentration possible, and the greater the resultant accomplishment. Each science is independent in so far as the individual investigator is concerned, and correlatively all sciences can be learned with each specific scientific problem as a point of departure, at least so far as the needs of that problem demand. On the solution of problems depends the future of science.

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RESEARCH IN INDUSTRIAL LABORATORIES[1]

At the second meeting of the Committee of One Hundred on Scientific Research of the American Association for the Advancement of Science, on December 28, 1914, the subcommittee on research in industrial laboratories was constituted to consist of Drs. R. F. Bacon (chairman), C. E. K. Mees, M. C. Whitaker, W. R. Whitney and W. H. Walker.

The following problems in the direction of industrial research have been considered by the subcommittee:

1. The organization of industrial research.

2. The selection and training of students for industrial research.

3. The factors involved in the promotion of cooperation between manufacturers and the universities, with particular attention to the depreciation of the policy of industrial secrecy.

4. The promotion of a better appreciation of research, with particular regard to the education of the public to the realizable functions of industrial research.

5. The establishment of stable relations between research institutions and the research departments of industrial plants.

6. Finally, the advisability of conducting a comparative study of the investigational activities, capacities and facilities of organizations devoted to or carrying on industrial research.

The conclusions arrived at by the subcommittee are presented in summary in the following report.

THE ORGANIZATION OF INDUSTRIAL RESEARCH

Principles involved in the organization of industrial scientific research have been discussed at length during the past year by Dr. C. E. K. Mees, a member of this subcommittee, in Science, N. S., *43*, 763. The chairman of the subcommittee has also considered some principles in the administration of endowed industrial research laboratories in the *Journal of the Society of Chemical Industry*, *35* (1916), No. 1.

It is generally conceded by those engaged in the direction of industrial research that, in order to be efficient, research laboratories of this type should be as thoroughly equipped as possible. In the case of industrial concerns having a number of plants and in the case of organizations of manufacturers, the tendency of organization should undoubtedly be towards concentration and cooperation in the maintenance of one large well-equipped research laboratory, rather than towards the erection and support of a number of smaller separated laboratories. It is, of course, necessary, especially in the case of chemical plants, that the analytical and control work be carried out *in situ*, but experience indicates that it is much better practise to centralize the research work.

Since the policy which insures adequate guidance to a research organization must be based upon the accumulation of facts, method in laboratory administration should provide for facilities for securing detailed information on a vast field, and for competent counsel from those who have a store of specialized knowledge. When the laboratory executive's work has passed the one-man stage, a division of labor comes about and it is here that he must see to it that he surrounds himself with men who are capable of effective effort—alert, original investigators of initiative and leadership.

An organized research administrative staff should not only result in effective division of labor, but also in efficient expenditure of executive energy, more effective plans, and general stabilization. This can come about if there is a pervading organization type of mind, which "is common to those drilled in systematic thinking and long immersed in the materials of their particular vocation. Such a mind sees details, but only as parts of a whole; reaches generalizations, but by the inductive route."

With regard to the investigatory staff, while the individual can exert only a very small influence except as a member of an organization or institution, yet a research institution never gains note or influence except through the

[1] Report of the Subcommittee on Research in Industrial Laboratories, presented by the chairman, Dr. Raymond F. Bacon, at the meeting of the Committee of One Hundred on Scientific Research, New York, December 26, 1916.

attainments and achievements of its individual members. The research department of a large industrial concern will be great because it has investigators on its staff who possess great originality and ability and because its director is wise and far-sighted. It is generally conceded that the personal factor is always paramount in industrial research, and that, as in every other organization, the control of men is the real problem in laboratory administration.

A brief consideration of the conditions favorable to both pure and industrial research is pertinent in connection with any discussion of the personal organization.

It is particularly adverse to progress to regard able investigators as abnormal men; for successful research demands neither any peculiar conformity nor any peculiar deformity of mind, but it requires, rather, peculiar normality and unusual industry and patience. It is little less inimical to expect productive work from those who are absorbingly preoccupied with other affairs than research; for fruitful scientific inquiry entails, in general, prolonged and arduous, if not exhausting, labor, for which all of the researcher's time is none too much. This is the experience of the Carnegie Institution and all other research organizations. It is only to be expected, therefore, that those most likely to produce important results in research are those who have qualified for the responsibilities thereof by the completion and publication of several worthy investigations, and who are at the same time able to devote the bulk of their energies thereto. The productive researchers in our universities are those who are devoting their whole time, or practically their whole time, to investigatory work.[2]

Research should never be allowed to fall into the rut of prosaic routine. The personnel of the investigatory staff should be maintained at the very highest standard and all administrative plans should be carried out with enthusiasm and earnestness.

In the research laboratories of manufacturing plants the personal cooperation of the research staff with the members of other branches of the organization always proves an important aid in maintaining interest in the work and is, in addition, mutually educating.[3] In particular, the research department should have an *esprit de corps* that keeps things moving and should lead the way so strikingly as to be apparent to all other departments of the corporation. In consequence, mediocrity should never be tolerated. It should be borne in mind, however, that the research man can only accomplish efficient work when he is free from restraint and petty annoyances.

Cooperation is always contributory to success in a research laboratory, and, other conditions being equal, the valuable men are the ones who can and will cooperate with one another. As in business, men succeed only as they utilize the ideas and services of other men. It follows, therefore, that the strength of an investigatory staff, properly operated, should increase more rapidly than the increase of its numbers, and that a fraternal spirit will play an important rôle in the productiveness of any research department.

The experience in several of our most successful industrial research laboratories has clearly shown that cooperation between the different departments thereof can be adequately and completely obtained by well-planned weekly conferences on the subjects under study. While some directors of industrial research hesitate to spend the time which these conferences entail, it is the opinion of the subcommittee that conferences of this na-

[2] As a rule, the head professors of chemistry in the larger universities are not giving more than three to five hours of lectures during the week, the rest of their time being devoted to research, while a number of them have one or more private research assistants, besides the candidates for advanced degrees, doing research work.

[3] In several of our largest corporations, the plant superintendents make monthly reports to the research departments, including all ideas of their own or of their assistants which may in any way warrant investigation. Then, too, the salesmen report regularly to the research department regarding the various ways in which the company's products are used and what substitutes are employed for the company's products. Such plans stimulate closer thought and observation.

ture are worth far more than the time they take.

THE SELECTION AND TRAINING OF STUDENTS FOR INDUSTRIAL RESEARCH

Research leading to the discovery of new ideas requires not only intellect and training, but also initiative or genius; it can come only from an individual who possesses unusual intuition and insight. It follows, therefore, that there is a scarcity of men gifted with the genius for industrial research and that it requires much experience in selecting suitable men and in training them to the desirable degree of efficiency, after having determined the particular qualities required.

The important requisites for industrial research are often unconsidered by manufacturers, who, in endeavoring to select a research chemist, are likely to regard every chemist as a qualified scientific scout. The supply of men capable of working at high efficiency as investigators is well below the demand; and chemists having the requisites and spirit of the researcher are indeed difficult to find even by those experienced in the direction of research. All research professors know that the location of a skilled private assistant—one who possesses not only originality, but also sound judgment and intellectual honesty—is not easy, because it frequently involves the gift of prophecy on the part of the searcher.[4] It has been truly said that the "seeds of great discoveries are constantly floating around us, but they only take root in minds well prepared to receive them."

On account of the extraordinary importance of new ideas, particular emphasis should always be laid upon finding and supporting brilliant researchers. Such individuals can best be found in the universities. The function of the university is to work with the beneficent idea of increasing the sum of human knowledge, and among its most valuable products are those who will work for the exercise of the investigative instinct and the pleasure of overcoming difficulties.

[4] See discussion in SCIENCE, N. S., 41 (1915), 319.

The examination of the training necessary for those proposing to take up industrial research which is common with all scientifically trained men, is too extensive a subject to be discussed by the subcommittee at this time. It is, however, appropriate to consider those subjects in which it seems desirable for the prospective researcher to specialize: reference is, of course, had to subjects other than those required by the average student of the sciences as distinguished from their industrial application, but the assumption is not made that what is desirable for research work should not also be available for all.

Research men frequently possess adequate training and scientific acumen, but fail in their ability to use it. There is no question that the element most noticeably lacking in the modern graduate is *resourcefulness.* A qualified research chemist who possesses initiative is usually a creator; but owing to the neglect of existing difficulties in chemical pedagogy, the present-day graduates of our schools of chemistry are too often deficient in inspiration, ingenuity and insight.

The failure to provide adequate and systematic instruction in chemical literature is illustrative of this contention.

Before commencing laboratory work upon any problem, it is obviously necessary to digest intelligently the important contributions which have been made upon the subject and to take advantage of what other workers have done in the same field. The average graduate is usually almost helpless when attempting to do this and consequently requires close supervision. The main difficulties are:

(*a*) He does not know how to go about it; he does not know where to look as the most probable source; and he is not familiar with the standard treatises and important journals.

(*b*) He fails to analyze the subject into its factors and hence generally looks for topics which are too general. Because he does not find any reference to the problem as a whole as he has it in mind, he assumes that nothing has been done upon it and that there is nothing in the literature which will be of aid to him in the investigation. Were he to sepa-

rate his subject into its essential parts and then to consult the literature on each factor, he would find considerable information which he otherwise would miss.

(*c*) He does not critically digest the articles under examination, but often he makes only a few disconnected quotations and fails to interpret the work done.

The solution is to be found in the provision in the chemical curriculum, preferably in the senior year, of a course of lectures on the literature of chemistry, with particular reference to the character of the writings and the status of the authors. The purpose of these lectures should be to present a general survey of the voluminous literature and to impart an accurate, systematic working knowledge of chemical bibliography. A concurrent seminar should be devoted to indexing and tracing chemical literature, to the cultivation of an acquaintanceship with authorities, and to the solution of bibliographic problems.

The subcommittee also recommends that pedagogic attention be given to the arrangement of a course of study in the principles of technical reporting and in the criteria of literary excellence in the preparation of reports of researches and professional reports. The completion of such a subject, with its accompanying analysis, practise and criticism, would usefully supplement the training received in chemical bibliography and would develop a capability which is much needed by chemical graduates.

It may be noted in passing that, during the academic year 1914–15, distinct courses in chemical literature and in technical reporting were established at the University of Pittsburgh. Much success has attended this pedagogic innovation.

The chemical graduate of to-day is also deplorably deficient in resourcefulness in planning research. While this is an extensive subject, a research student may be trained in correct methods of attack, namely:

(*a*) *Analytical Methods.*—Almost all investigations require analytical control. In no feature of chemical work is there more apparent an inability to use the analytical training which the man has received.

(*b*) *Planning the Investigation.*—Resourcefulness in separating a problem into its essential factors and in clearly grasping the interrelationship of these factors is most important. Too many men desire to start in at once and solve the problem at the first attempt. All this might be summed up in the expression "methods of research."

(*c*) *Apparatus.*—The subcommittee has not considered just how a man could be trained to be more resourceful in this respect, but it is surely a marked weakness in the average graduate. While a native cleverness is doubtless born, and not made, it ought to be possible to give the undergraduate some training in the use of his mental equipment in designing and planning apparatus which is to accomplish the desired end.

THE FACTORS INVOLVED IN THE PROMOTION OF COOPERATION BETWEEN MANUFACTURERS AND THE UNIVERSITIES[5]

The recent impetus imparted to the research activities in American chemical manufacturing has materially altered the traditional policy of industrial secrecy. A striking illustration of this improvement is to be found in the reports of the Industrial Conferences held at the fifty-third meeting of the American Chemical Society.[6] This change in attitude, a natural result of the appreciation of urgent action in industrial research, has long been desired by our universities and it will undoubtedly result in the extension of the practise of referring certain of the problems of industry to university laboratories for study. Many of the numerous problems of chemical as well as mechanical technology could be advantageously attacked outside of the plants, but some central organization is needed for securing and properly distributing those problems which are pressing. It is clear, however, that stable

[5] The president of the American Chemical Society has been authorized to appoint a central committee from representatives of the universities and the industries to study opportunities and to make recommendations for cooperation.

[6] See *J. Ind. Eng. Chem.*, 8 (1916), 947 *et seq.*

relations between the universities and industrialists will be worth while only if some mutual benefit can accrue therefrom. This cooperation can therefore be most satisfactorily promoted by actively demonstrating the advantages of the exchange or interchange of subjects for research, which primarily presupposes a reasonable freedom from the concealment of knowledge which persistently adheres to all industrial research.

Industrial research laboratories can be of mutual aid by supplying advice and materials. These laboratories should also publish reports of investigations just as freely as possible and thus, by proving the utility of it, assist in the general scheme of the universities—promote the dissemination of knowledge.

In general, the subcommittee endorses the conclusions of the University and Industry Committee of the New York Section of the American Chemical Society.[7]

THE PROMOTION OF A BETTER APPRECIATION OF RESEARCH

The promotion of a better appreciation of research by the general public can only be obtained by publicity.[8] No complaint can be made of a lack of this at the present time. The large corporations supporting industrial laboratories are themselves expending great sums on giving publicity to their research work. The subcommittee thinks, however, that though the general public now appreciates the value of scientific research, the thing required to increase the number of laboratories is more information as to specific plans for starting and running them. General articles on the advantages of research work would be very much helped in carrying conviction if they were accompanied by definite proposals telling manufacturers of different industries and of different grades in the size of their work what they could do in the way of research work themselves.

The average person who has to decide whether his corporation will support research work can, in the nature of things, know little about it. He desires either to spend much less than is necessary for effective work or he is frightened by the size of the expenditure which he thinks will be necessary. More specific information would enable him to form a truer idea as to what he was committing himself and what he was likely to get.

As far as possible, arrangements should be made for research institutions to have information as to their work available and to persuade them to give this information freely to inquirers. It would be a considerable step in cooperative effort if all the research institutions that can be reached could be persuaded to put information regarding themselves into some form so that a comparison could be made.

THE ESTABLISHMENT OF STABLE RELATIONS BETWEEN RESEARCH INSTITUTIONS AND THE RESEARCH DEPARTMENTS OF INDUSTRIAL PLANTS

The suggestion has been frequently made that the establishment of stable relations between the types of organizations mentioned might be effected if a small group of selected representatives thereof could arrange to confer at regular times. After consideration, the subcommittee recommends the formation of an association of research institutions, that is, an association of all those bodies engaged in scientific and scientific industrial research, including such organizations as the research laboratories of Harvard University, the Massachusetts Institute of Technology, and other educational institutions, the Carnegie Institution laboratories, the Mellon Institute of Industrial Research, and the research laboratories of the corporations which are conducting a certain amount of research of scientific importance. Undoubtedly, an association of this nature would meet with satisfactory support and it would eventually prove an important factor in improving the methods of research organization.

[7] See *J. Ind. Eng. Chem.*, 8 (1916), 658.

[8] It is important to mention here that the American Chemical Society has under consideration the publication of a journal of popular chemistry, a periodical for which there is a real need because of the desirability of the proper dissemination of chemical information to the public.

Stable relations between various research organizations will be worth while only if some mutual benefits can accrue. These can be brought about by an exchange or interchange of "commodities," such as—

(*a*) Subjects for research.

(*b*) Special facilities for extraordinary conditions, such as extreme pressures, extremes of temperatures, etc.

(*c*) Special pieces of expensive apparatus.

(*d*) Helpful ideas on research already in progress.

(*e*) Candidates for employment.

This presupposes a freedom from the secrecy which still surrounds the industrial research of certain organizations. Undue secrecy is unnecessary and unwise, but it is only in those cases where publicity is compatible with industrial progress that full cooperation between the universities and the industries can be effected.

A COMPARATIVE STUDY OF INVESTIGATIONAL ACTIVITIES

This study would be distinctly worth while, but before the initiation of such a movement there must first be established more mutual confidence than now exists. A comparative study of this kind would be very difficult and would necessitate the expenditure of much time. Probably such information could be secured by obtaining the reports regarding the industrial research laboratories in operation, and there is no reason why a suitable questionnaire could not be prepared and distributed, in order to obtain information regarding research conditions and comparative data relating to the organizations maintaining laboratories.

It would be very useful indeed to have available a yearbook pertaining to research laboratories, with the following lines of information: institutions, organizations or concerns supporting them, approximate purpose of laboratory, divisions of science represented therein, manufacturing facilities directly associated therewith, approximate annual expenditure for maintenance of research, number of and particulars relating to the training of the members of the investigatory staff, and, finally, a list of the scientific publications for the past year. Such a book might also advantageously include mention of the special equipment of the laboratories unlikely to be possessed by every similar institution.

The National Research Council, through its committee on research in educational institutions, could well arrange to have some one whose sole duty it was to coordinate the work in university laboratories with reference to general or national welfare. While any attempt which may be made by a national society or association to secure cooperation between industrial and institutional laboratories will invariably encounter the difficulty of invested interests, an organization with governmental support might accomplish much fruitful research work through institutions of learning and in such a way that this would be of material benefit to the institutions concerned, as well as to the nation.

R. F. BACON,
Chairman,
C. E. K. MEES,
W. H. WALKER,
M. C. WHITAKER,
W. R. WHITNEY

PITTSBURGH, PA.,
December 15, 1916

SCIENTIFIC EVENTS

THE CONTROL OF TUBERCULOSIS IN FRANCE

GOVERNOR WHITMAN, of New York, has granted Dr. Hermann M. Biggs, state health commissioner, leave of absence to go to France, at the request of the Rockefeller Foundation, to conduct an organized campaign to combat the spread of tuberculosis among noncombatants. In a letter to Governor Whitman, Mr. Jerome D. Greene, secretary of the foundation, wrote:

> For some time past our representatives in France have been much impressed by the need of effective measures for the relief and control of tuberculosis. A number of voluntary American agencies in France have exerted themselves with great zeal to arouse the sympathy of the American public and to do what could be done to provide hospital care for the more urgent cases that have

come under observation. A committee of French citizens has also been organized in close cooperation with the government, and appeals have been made to us on behalf of the work undertaken by this committee.

In response to the appeals we have received, our representatives have made a careful preliminary study of the situation, and the trustees of the Rockefeller Foundation have been so impressed with the gravity of the need that they have decided to take steps to ascertain definitely the lines along which American sympathy and generosity can be made most effective. With this end in view, they have sought to find the man who in all the country was best qualified both as a physician and as a public health administrator to study the situation in France and to determine the lines along which help could best be given.

They have had no difficulty in making up their minds that Dr. Herman M. Biggs was the man whose character and attainments best fulfilled the requirements of the case. They realized that it would be asking a great deal of Dr. Biggs to make the sacrifice involved in a visit to France, and that the state of New York had the first claim on his services. They felt, however, that if it should be the happy result of Dr. Biggs's going to France that the benefits of his long and wonderfully fruitful service in New York could be availed of in the organization of the campaign in that country, the effect in terms of human welfare would be so large and far-reaching as to constitute a very strong claim both on his public spirit and upon the generosity of the state of New York.

During Dr. Biggs's absence Dr. Linsly R. Williams, deputy commissioner, will be acting commissioner; Dr. Matthias Nicoll, Jr., now secretary of the board, will be acting deputy commissioner, and Dr. John A. Smith, at present sanitary supervisor, will act as secretary.

THE NATIONAL PARKS CONFERENCE

UNDER the auspices of the National Park Service of the Department of the Interior there was held in the auditorium of the New National Museum, Washington, D. C., on January 2, 3, 4, 5 and 6, 1917, a National Parks Conference, at which many important papers and lectures were presented. The program included:

Our National Parks: Franklin K. Lane, secretary of the interior; Senator Reed Smoot, of Utah; Representative Scott Ferris, of Oklahoma; Representative Irvine L. Lenroot, of Wisconsin; Carl Vrooman, assistant secretary of agriculture; Enos Mills.

Canadian National Parks: J. B. Harkin, commissioner of Dominion Parks, department of the interior, Canada.

The Public and the National Parks: Huston Thompson, Jr., assistant attorney general.

University Classes in the National Parks: Professor E. M. Lehnerts, of the University of Minnesota.

Public Schools and the National Parks: Philander P. Claxton, U. S. Commissioner of Education.

National Parks as a Scientific Asset: Dr. Charles D. Walcott, secretary, Smithsonian Institution.

Teaching by Picture: Gilbert H. Grosvenor, editor, *National Geographic Magazine.*

The Painter and the National Parks: William H. Holmes, head curator, National Gallery of Art.

The Photographer and the National Parks: Fred H. Kiser.

National Forests and National Parks in Wild Life Conservation: Henry S. Graves, forester and chief, Forest Service.

The Yellowstone Elk Herds: E. W. Nelson, chief of the Bureau of Biological Survey.

Future of the Antelope: E. Lester Jones, superintendent, Coast and Geodetic Survey.

National Monuments as Wild Animal Sanctuaries: T. S. Palmer, assistant in charge of Game Preservation, Bureau of Biological Survey.

Colossus of Canyons: Representative Simeon D. Fess, of Ohio.

The Survey's Contribution to the National Park Movement: Dr. George Otis Smith, director, U. S. Geological Survey.

The Problem of the Greater Sequoia: Representative Frederick H. Gillett, of Massachusetts.

Perhaps Our Greatest National Park: Enos Mills.

The Tehipite Valley and Kings Canyon: Robert Sterling Yard.

The Top of America—Mount Whitney: Emerson Hough.

A FRENCH NATIONAL PHYSICAL LABORATORY

THE question of national laboratories of scientific research has been brought forward recently in France. In the *Comptes rendus* of the Academy of Sciences for November 13, as summarized in *Nature,* is a preliminary

report by a committee composed of MM. Jordan, Lippmann, Emile Picard, d'Arsonval, Haller, A. Lacroix, Tisserand and Le Chatelier on this question. It is pointed out that all the great industrial nations possess national laboratories of scientific research, systematically directed towards the study of technical problems. The National Physical Laboratory in England, the Bureau of Standards and the Carnegie Institution in the United States, the Physikalische Reichsanstalt and the institutes founded by the Wilhelm Gesellschaft in Germany are given as examples. France has no corresponding institution, and after a full discussion of the questions of control, staff, and work to be done, the following resolution was unanimously carried:

> The Academy of Sciences, convinced of the necessity of organizing in France, in a systematic manner, certain scientific researches, expresses its wish that a National Physical Laboratory should be started, for the prosecution of scientific researches useful to the progress of industry. As in other countries, this laboratory would be placed under the control and direction of the Academy of Sciences.

On November 27 this question was further considered by the academy, and it was suggested that the general direction of the laboratory should be entrusted to a council, one half of the members to be nominated by the academy, one quarter representatives of the state departments, and the remaining quarter delegated by the principal industrial interests. Certain existing state laboratories might be affiliated to the national laboratory. A considerable grant for establishment and maintenance will be necessary.

DEDICATION OF THE NEW YORK STATE MUSEUM

ALTHOUGH the New York State Museum at Albany has been open to the public for some months past, it seemed wise to the regents of the university to bring the public into closer touch with the new museum by formal dedicatory exercises. These took place in the chancellors' hall of the education building at Albany on the afternoon and evening of Friday, December 29. The afternoon exercises consisted of a series of addresses from eminent speakers, each representing a special phase of community interest in the museum. The Honorable Charles B. Alexander, chairman of the regents committee of the State Museum, presided, and the speakers were President John H. Finley on behalf of the university and the educational system of the state; Senator Henry M. Sage on behalf of the state government; Doctor Francis Lynde Stetson on behalf of the people; the Honorable Charles D. Walcott, speaking as a representative of science in its broadest sense, and Director John M. Clarke on behalf of the museum.

In the evening the principal address was by Colonel Theodore Roosevelt, who spoke under the title "Productive Scientific Scholarship," and gave an interesting speech to a large audience. Colonel Roosevelt was introduced by Governor Charles S. Whitman, who very happily set forth the value of the research work of the scientific corps attached to the museum. The evening exercises were felicitous and successful throughout, and were followed by a reception in the halls of the museum. Colonel Roosevelt's address on this occasion, or the part of it that related especially to his scientific theme, has been already printed in SCIENCE, and all the addresses of the occasion will be published as a bulletin of the university.

SCIENTIFIC NOTES AND NEWS

PROFESSOR FRANK D. ADAMS, of McGill University, has been elected president of the Geological Society of America. Dr. Charles P. Berkey, of Columbia University, continues as acting secretary, in the absence in the Arctic regions of Dr. E. O. Hovey.

OFFICERS of the Mathematical Association of America elected at the New York meeting, on December 29, are: *President,* Florian Cajori, Colorado College; *Vice-presidents,* Oswald Veblen, Princeton University, and D. N. Lehmer, University of California; *Secretary-treasurer,* W. D. Cairns, Oberlin College; *Members of the Council* to serve until January, 1920: E. R. Hedrick, University of Missouri; Helen A. Merrill, Wellesley College; R. E. Moritz, University of Washington; D. E.

Smith, Columbia University; *Member of the Council* to take the place of Florian Cajori (elected president): E. V. Huntington, Harvard University.

Dr. F. W. Taussig, professor of political economy at Harvard University, is reported to have accepted the chairmanship of the tariff commission created by the present congress.

The title of emeritus professor of physics in the University of London has been conferred by the senate on Dr. F. T. Trouton, who held the Quain chair of physics until 1915.

Dr. A. Yersin, director of the Pasteur Institute of Indo-China, has been awarded the Lasserre prize for the present year for his work on anti-plague serum.

Privat-Dozent J. Kyrle, of the University of Vienna, has been awarded $200 by the Austrian Academy of Sciences to continue his experimental researches on leprosy.

Mr. William Grunow, eighty-seven years old, who was for thirty-six years custodian of the United States Military Academy Observatory at West Point and a skilled instrument-maker, died on January 5.

A correspondent writes that Mr. Orville Wright has moved into his recently completed laboratories at Dayton, Ohio. The death of Wilbur Wright a year ago caused the suspension of work on the problems of aviation for a time. But in November Mr. Wright resumed flying at his aviation field and dropped his experiments only when the winter weather interfered. Mr. Wright states that there are certain experiments having to do with the theoretical side of aeronautics which the Wright brothers had made prior to 1905. They gave up experimentation for flying. One of the experiments with which Orville Wright will busy himself is the wind funnel. He began observing the effect of wind currents on plane surfaces early in the year. As soon as spring comes, Mr. Wright will begin flying again at his aviation field and will continue his experiments in the new laboratories.

Professor Frederick E. Breithut, of the department of chemistry of the College of the City of New York, has issued a report to the New York Section of the American Chemical Society, urging a statistical investigation of the chemists of the United States so that the conditions of employment and opportunities for young men entering the profession may be ascertained. The committee, appointed by Dr. J. Merrit Matthews, chairman of the New York Section, consists of Professor Frederick E. Breithut, chairman; Elwood Henrick, Bernhard C. Hesse and Otto H. Klein.

Dr. Richard M. Pearce, the John Herr Musser professor of research medicine in the University of Pennsylvania and adviser in medical education to the International Health Board of the Rockefeller Foundation, sails on January 15 for Argentina and Uruguay to study medical conditions in these countries.

A grant of $250 has been made by the C. M. Warren Committee of the American Academy of Arts and Sciences to Professor E. L. Mark, of Harvard University, for the investigation of certain properties of sea water at the Bermuda Islands.

The Association of Military Surgeons of the United States has announced the results of the Henry S. Wellcome prize competition. Capt. Mahlon Ashford, of the Army Medical Corps, who wrote on "The Organization of Medical Officers," was awarded a gold medal and $300. A silver medal and $200 was awarded to Assistant Surgeon-General W. C. Rucker, of the Public Health Service, whose essay was entitled: "The Influence of the European War on the Transmission of the infections of Disease."

Under the Herter Foundation the faculty of the University and Bellevue Hospital Medical College announces five lectures by Professor Archibald B. Macallum, of the University of Toronto, on "The Distribution of Inorganic Compounds in Animal and Vegetable Tissues and the Forces that determine it." These lectures began January 8, at 4 o'clock, at the Carnegie Laboratory, and will continue daily at the same hour.

Dr. Forest Ray Moulton, professor of astronomy in the University of Chicago, will give in February, five lectures at Western Reserve University on the MacBride Foundation.

WE learn from *Nature* that a fund is being raised to purchase the very valuable scientific library of the late Professor Silvanus Thompson and to present it to the Institution of Electrical Engineers as a memorial of his life and work, the library to be accessible to the public on the same conditions as the Ronalds Library. Those who wish to subscribe to this fund or to have further information regarding it are requested to communicate with Mr. W. M. Mordey, 82 Victoria Street, London, S.W.

A CORRESPONDENT writes: "In the death, on December 2, of Dr. Herbert Armistead Sayre, professor of mathematics and sometime professor of physics at the University of Alabama, there passed a true gentleman, and thousands of his past students will ever keep within their hearts a warm appreciation of his kindly friendliness and sterling worth."

THE death is announced of Mr. William Ellis, F.R.S., in his eighty-ninth year. Mr. Ellis was formerly superintendent of the Magnetical and Meteorological Branch of the Royal Observatory, Greenwich. He joined the Observatory in 1841, and was attached to the astronomical department until 1874, having during the preceding eighteen years been in charge of the chronometric and electric branch.

DR. RAYMOND TRIPIER, former professor of pathologic anatomy at the Lyons School of Medicine, has died at the age of seventy-eight years.

WORD has come to this country of the death on the battlefield at Artois, France, on September 15, 1915, of Dr. Bernard Collin, of the staff of the zoological station of the University of Montpelier, situated at Cette. Dr. Collin had made a brilliant record by his researches on the cytology of the Suctoria and was regarded as one of the most promising of the younger protozoologists of France.

UNIVERSITY AND EDUCATIONAL NEWS

THE University of Cincinnati has received by the will of Francis H. Baldwin, of Cincinnati, a bequest of approximately $675,000 for the unspecified uses of the university.

JEFFERSON MEDICAL COLLEGE at Philadelphia has received a gift of $150,000 from Miss Anna J. Magee to endow the Magee professorship for the practise of medicine and clinical surgery, now held by Professor Thomas McCrae. During the year, the college has received $100,000 from Daniel Baugh to establish the provost professorship of therapeutics, held by Dr. Hobart A. Hare, and an equal sum from friends of the college to endow the Samuel D. Gross professorship of surgery, held by Dr. J. Chalmers Da Costa. It is understood that these gifts are intended to make unnecessary the merging of the Jefferson Medical College with the Medical School of the University of Pennsylvania.

THE board of trustees of the Emma Willard School, Troy, N. Y., announces that, continuing her benevolence toward this school and the new Russell Sage College of Practical Arts, Mrs. Russell Sage has given $250,000 toward the advancement of the work of this latter institution, the only requirement being that the money so given should be used the same way as the original gift of a like amount a year ago to establish the college.

FRIENDS of the University College of Wales, Aberystwyth, have expressed their intention of contributing £100,000 to the funds of the college, subject to a reservation of their right to make proposals to the council as to either the capital or the income.

HAROLD VEATCH BOZELL, E.E., director of the University of Oklahoma Electrical Engineering School, is temporarily serving on the Sheffield Scientific School faculty of Yale University. Dr. Alois Francis Kovarik has been appointed assistant professor of physics in the school.

MR. F. R. GRIFFITH, JR., A.M. of Washington University, who has been research assistant in physiology at Tulane University, has been elected assistant professor of biology at Southern Methodist University, Dallas, Texas.

DR. H. A. L. FISHER, vice-chancellor of Sheffield University, has been appointed president of the Board of Education in the new British cabinet.

DISCUSSION AND CORRESPONDENCE

A CASE OF SYNCHRONIC BEHAVIOR IN PHALANGIDÆ

A recent article in this journal by Wallace Craig on "Synchronism in the Rhythmic Activities of Animals" recalls to mind an observation that I made near Austin, Texas, in 1909. At the time of the observation I made some field notes from which the following description is taken.

While engaged in hunting various species of rock lizards I located a vast colony of "harvestmen," which I identified as belonging to the genus *Liobunum,* resting during the day on the under side of an overhanging shelf of rock on a precipitous hillside. In a somewhat circular area of nearly five feet in diameter the harvestmen were packed closely together in almost unbelievable numbers. I estimated that there were between one and two thousand in the colony. When I first saw them they were all hanging from the ceiling, as it were, perfectly motionless, but when I came within about six feet of them they began a curious rhythmic dance. Without changing their foot-holds they raised their bodies up and down at the rate of about three times a second, and, curiously enough, the movement of the entire lot was in the most perfect unison. This performance was kept up for over a minute and then stopped gradually as though from exhaustion. I then poked a few of the nearest individuals with a stick and these immediately resumed the rhythmic up-and-down movement, which spread quickly over the whole group, but died down in less than half a minute. When I once more stirred up a few individuals they gave a few rhythmic responses, which stirred the whole colony again, but only slightly. After this a number of individuals began to crawl about and it was no longer possible to stimulate the rhythmic behavior.

When the colony was first seen it was noted that the long legs of neighboring individuals were closely interlocked and this mechanism was sufficient to account for the transmission of stimuli from one part of the colony to another. It should be noted especially that the rhythm was not perfectly synchronous at the beginning, but became so after a few seconds.

Possibly synchronic flashing in fire-flies may be explained as the result of a somewhat similar transmission of stimuli. One flash stimulates others, which at first might lag slightly; but soon a synchronism is built up in a limited region, such as one bush or one tree. Such a synchronism might be transmitted to a whole field.

It would be interesting to know whether any other naturalist has observed the type of behavior herewith described for the Phalangidæ.

H. H. Newman

University of Chicago

THE SUPPOSED SYNCHRONAL FLASHING OF FIREFLIES

I was very much interested in reading the article by H. A. Allard, entitled "The Synchronal Flashing of Fireflies," which appeared in Science, November 17, 1916. Some twenty years ago I saw, or thought I saw, a synchronal or simultaneous flashing of fireflies (Lampyridæ). I could hardly believe my eyes, for such a thing to occur among insects is certainly contrary to all natural laws. However, I soon solved the enigma. The apparent phenomenon was caused by the twitching or sudden lowering and raising of my eyelids. The insects had nothing whatsoever to do with it. Many times in the past twenty years I have proved that my solution was correct.

Philip Laurent

TRIMMED MAGAZINES AND EFFICIENCY EXPERTS

To the Editor of Science: I have been reading your article on page 13 of Science for January 5 entitled "Science and the Cost of Paper" and am very sorry that the price of paper has increased to such an extent that you have to make a material change in Science. I understand your position and am not objecting the slightest to what you are doing; but I do want to make a protest against this popular efficiency humbug, because it seems to me that people are running the efficiency matter into the ground. It's all nonsense for any efficiency expert to say that the opening

of SCIENCE by hand cost scientific men $10,000 per year. Of course it might if men sat down and opened the magazine and then afterwards read it through, but I have always found that I got more out of an unopened magazine than an opened one, because I would more carefully examine a magazine that I had to open than one that was opened; because, as I opened it, I either read the magazine, or if I didn't want to read the articles, got a rough idea of them as I opened the magazine, and for that reason whenever possible I try to get an unopened magazine.

We are losing in this nonsense regarding efficiency a good deal of the human interest in men in our employ and it's a great question to my mind if efficiency is not doing more damage than good. H. P.

[The editor shares to a certain extent his correspondent's prejudice against trimmed magazines and efficiency experts. An untrimmed journal looks as if it were waiting for careful reading and the binder; a trimmed one for a hasty glance and the waste-paper basket. This, however, is a matter of association, which is already changing with general usage. Trimmed magazines and efficiency experts have apparently arrived. We must get used to the one and treat the other with discretion.]

SCIENTIFIC BOOKS

Die Kultur der Gegenwart. Herausgegeben von PAUL HINNEBERG. Teil III., Abtlg. III. Physik, S762. Teubner, 1915.

During the past two years much has been written about Kultur. There has been a tendency in the English-speaking world to identify it with "culture," a term which with us is variously defined. While our dictionaries may give as the equivalents of culture the following: knowledge, development, the training of the mind, the intellectual side of civilization —the more common use of the English word is associated with refinement, taste, manners. It is this common meaning which leads Stephen Leacock to speak of a cultured man as "one who has acquired a silk hat and the habit of sleeping in pyjamas." Associating culture with refinement we generally think of it as denoting knowledge of fine arts, of music, literature, languages, especially *ancient* languages. Indeed, John Bright complained that the only necessary qualification of a cultured man was that he possess a *smattering* of two *dead* languages, Latin and Greek. Gradually, however, we are getting away from identifying culture with a knowledge chiefly of languages, living, dead and half dead, with taste and manners, and are coming to view it as "that complex whole which includes knowledge, belief, art, morals, law, custom and any other capabilities and habits acquired by man as a member of society." In this view we are approaching the idea of "Kultur" as set forth in this volume and its companions in the series. In passing it should be noted that even among Germans there are a variety of views concerning "Kultur." Professor Münsterberg defines it as "the consciousness of nationalism, the subordination of the individual to the national ideal." But if one desires to ascertain the meaning of "Kultur" as here set forth, one should read the 760 pages of this volume which is concerned only with physics. One then should survey the contents of the other fifty-seven volumes of the "Kultur" series.

The fifty-eight volumes comprising "Die Kulture der Gegenwart" are divided as follows: fourteen are devoted to religion, philosophy, literature, music, art; ten to history, economics, the political and social sciences; nineteen to mathematical, natural and medical sciences; fifteen to technical sciences.

In the volume under review there is presented the philosophical evolution rather than the history of physics. *Ideas* are traced from their origin to their present fullness. One is thus able to observe how the contributions of the succeeding centuries and decades compare with one another. It is interesting to note that in the article on mechanics, which may be regarded as the *oldest* portion of physics, thirty-six pages suffice to bring the subject to near the end of the nineteenth century and twenty-five pages are given to the development during the past generation. In the other thirty-five articles thirteen are almost en-

tirely concerned with the developments of the past twenty-five years and the other articles are largely taken up with such development. Obviously physics is a new subject and Kultur has grown in recent years.

The thirty-six articles in this volume are contributed by thirty-two specialists, all Germans with two exceptions—H. A. Lorentz and P. Zeeman. The assignment of subjects to writers is, with perhaps a couple of exceptions, that which an American physicist would make. Thus H. Rubens writes on Wärmestrahlung; W. Wein on Theorie der Wärmestrahlung; Dorn on Experimental Atomistik; Einstein on Theoretische Atomistik and Die Relativitätstheorie; Lorentz on Die Maxwellsche Theorie and Die elektronen Theorie; P. Zeeman on Magnetooptik; Lecher on Die Entdeckungen von Maxwell und Hertz; Braun on Die Drahtlose Telegraphie; Kaufmann on Die Kathodenstrahlen und Die Röntgenstrahlen; Max Planck on Das Prinzip der kleinsten Wirkung und Verhältniz der Theorie zueinander. We are rather surprised to see that the article on Die Positiven Strahlen is given by Gehrcke and Reichenheim rather than by Starke. To the latter is assigned Das Elektrische Leitungsvermögen. A detailed and technical review of this large work is here impossible, but certain general characteristics may be noted.

The authors treat their respective topics in a general and philosophic manner. One rather striking point is that, except in an occasional appendix and except as they are required for the condensed statement of a physical law, mathematical equations do not occur. The reader of this volume will not be troubled by analytical difficulties. In this respect the articles in this volume offer a striking contrast to those on similar topics in the Encyclopædia Britannica.

All of the material contained in these articles can be found scattered through the earlier handbooks or the recent scientific periodicals. The criticism may therefore be made that this volume tells a physicist nothing with which he is not already acquainted, while it is of no value to one who is not a physicist, by reason of the fact that the latter would be unable to follow the condensed argument. This criticism, however, is rather forced. Many physicists who have been carrying on intensive work in limited fields will find here a very acceptable summary of those portions of the subject with the literature of which they have not been able to keep in touch.

As the contributors of the articles in this volume are almost all Germans, it is natural that the work of German physicists should receive adequate, perhaps unduly favorable, recognition. Thus Helmholtz is given credit for originating the present electron theory through the emphasis he placed upon Faraday's statement concerning the absolute quantity of electricity associated with the particles or atoms of matter. Probably most physicists have come to the conclusion that Faraday stated for electrolysis in as clear language as it could be stated the law of multiple proportions of electricity and matter.[1] Helmholtz had no more experimental evidence than had Faraday to extend this law beyond the field of electrolysis. It required the work of Schuster, Perrin, Kaufmann, Wilson, J. J. Thomson, to complete the statement. If we are going to give credit along this line to any one between Faraday and these later workers, we should not lose sight of the fact that in 1871 before the British Association, Johnstone Stoney spoke of the quantity of electricity appearing in electrolysis as the *natural unit* and later gave to it the name electron—a name which has been retained.

Ordinarily the writers of articles have not overstated the importance of their own contributions, but in the article on Wireless Telegraphy by Ferdinand Braun full justice is done to the author's experimental work.

Passing from electricity to thermodynamics one is surprised to see even in a minor paragraph the name of Holtzmann preceding that of Joule in connection with the determination of the mechanical equivalent of heat. We know that Holtzmann by his work on the specific heat of gases made a contribution of some importance and that he computed the mechanical equivalent of heat—but his ideas were hazy as to the nature of heat, for

[1] See "Experimental Researches," Vol. I., par. 852.

at times he assumed, as did Carnot, that the quantities of heat entering and leaving the cylinder of an engine, irrespective of the performance of work, were equal. If for the sake of completeness Holtzmann's work be referred to in this volume, so too should that of Marc Léguin (1839), who as far as the performance of an engine is concerned anticipated other workers (except possibly Rumford and Davy) in a partial statement of the law of the equivalence of heat and work.

Had this volume been written by American physicists, emphasis would have been placed on parts of the subject not here noticed. Wood on resonance spectra, Nichols and Merritt on fluorescence, Miller, Webster, Sabine on sound, Michelson on the rigidity of the earth, Pupin, G. W. Pearce on wireless telegraphy, would have been recorded.

But when we come to view the great body of philosophical thought which has come to us in this past generation we must give to Teutonic physicists credit for a large share. Boltzmann's conception of the entropy of a body in terms of the probability of state; the extension by Planck of the idea of entropy and temperature to radiation, leading to the distribution of energy in the spectrum of a full radiator and to the bewildering quantum theory; Einstein's contributions to molecular theory and to the theory of relativity—these stand out as substantial portions of "Die Kultur der Gegenwart."

G. F. HULL

SPECIAL ARTICLES

PEANUT MOSAIC

ON September 28, 1915, while looking over a field in which peanuts (*Arachis hypogœa*) had been grown annually for the past six years a plant was observed, one shoot of which bore mottled leaves. A careful search of the entire field was made, but no other plant bearing mosaic leaves was found. This made the writer suspect that the trouble was not infectious. It seemed advisable to test this point further, especially since the mosaic plant was otherwise healthy except for a few leaf spots produced by *Cercospora personata.*

This mosaic plant was transferred to the greenhouse. Before final potting two of the mature pods were removed from the plant and opened, and four peas taken from them were planted at once in a pot of greenhouse soil. The four resulting plants together with two other seedlings which came up later from peas left on the mosaic plant, have been under observation during the past five months. In no case have any signs of mosaic developed. It would thus appear that this mosaic was not carried by the seed.

The transplanted mosaic plant continued to grow and produce new leaves at the ends of the shoots, but in no case did any but the mosaic shoot produce new mosaic leaves.

To obtain further data as to the infectious nature of this mosaic a pot of four peanut plants from a 1914 crop of seed was selected. Two plants were slashed near the ends of the shoots with a flamed scalpel to serve as checks. The other two plants were treated in a similar way, except that into the slashed stems bits of macerated mosaic leaflet were inserted. These plants have been under observation for the past five months but no signs of mosaic have developed on either the checks or inoculated plants.

On October 14, 1915, a pot containing peanut plants from the 1914 seed was taken to the laboratory. By means of India ink circular areas were marked on each leaflet of one plant. Within these circles the tissues were pierced several times with a flamed dissecting needle. This plant served as a check. The second plant in the same pot was treated in a similar way except that before piercing the leaf tissues the needle was moistened in the juice from mosaic leaflet freshly removed from the potted mosaic plant. Similar checks and inoculations were made on garden peas (*Pisum* spp.) growing in pots, using juice from the mosaic peanut leaflet. On November 13, 1915, the above plants were carefully examined, but neither the checks nor the inoculated plants showed any sign of mosaic on either young or old leaves.

On November 13, 1915, to further test the infectious nature of this peanut mosaic one check was prepared by injuring each leaflet of the plant by pinching it between the thumb

and finger nail. Eight other plants of the same age and all from the 1914 crop of seed were treated similarly except that the finger nail was moistened in macerated mosaic leaves before pinching each leaflet to be inoculated. Over three months have elapsed since the above inoculations were made, but no signs of mosaic have developed on any of the checks or on the inoculated plants. On all the leaves, however, the scars of the finger nail injury are visible.

As the original mosaic plant has matured in the meantime, leaving no fresh leaves to use for inoculation, it seems advisable to present this data so that others may be led to record any observation they may make along this line.

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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

SECTION C—CHEMISTRY—AT THE NEW YORK MEETING

On Wednesday, December 27, at Columbia University, there was a joint session of Sections B and C, the American Chemical Society and the American Physical Society, devoted to a symposium on the structure of matter. The attendance was very large, and Havemeyer Hall was filled to capacity. The main items on the program have already been printed in a recent number of Science.[1] These papers[2] and the subsequent discussion brought out the fact that there is still a wide divergence between the various views, particularly between those of the physicist and those acceptable to the chemist; the mere fact of such a divergence of view emphasizes the usefulness of this discussion—and, indeed, of further discussion—of this very important topic.

On Thursday, December 28, Section C met with the American Chemical Society and the Society of Chemical Industry at the College of the City of New York, when the following addresses were presented:

Dr. William McPherson, retiring chairman of Section C, professor of chemistry, Ohio State University, "Asymmetric Syntheses and their Bearing upon the Doctrine of Vitalism."

Dr. Phoebus A. Levene, Rockefeller Institute for Medical Research, "The Individuality of Tissue Elements."

Dr. Hugh S. Taylor, Princeton University, "The Photo-Chemistry of the Chlorination Processes."

Dr. George F. Kunz, New York City, "Preparedness Chemistry Exhibit of the United Chemical Societies at the American Museum of Natural History."

Dr. C. G. Derick, Buffalo, "Equilibrium Constants and Chemical Structure."

Dr. S. Dushman, Schenectady, "Application of Atomic Theories in Chemistry."

Through the courtesy of the college, a complimentary luncheon was tendered to the section, which was highly appreciated. In the afternoon the following papers were read:

A Preliminary Report of the Chemical Committee of the National Research Council, by Marston Taylor Bogert, chairman.

"An Increase in the Sucrose Content of Sugar Beets after their Removal from the Soil," by F. G. Wiechmann.

"Valency and Valence," by M. L. Crossley.

"Conductivity Measurements on Oxidation-Reduction Reactions," by Graham Edgar.

"Stability of Paraffin Hydrocarbons," by G. Egloff and R. J. Moore.

The following by title only:

"The Effect of Fineness of Division of Pulverized Limestone upon Various Crop Yields," by N. Kopeloff.

"A Relation between the Chemical Constitution and the Optical Rotatory Power of the Phenylhydrazides of Certain Acids of the Sugar Group," by C. S. Hudson.

"*d*-Mannoketoheptose. A New Sugar from the Avocado," by F. B. La Forge.

Section C elected new officers, as follows:

Vice-president and Chairman of the Section: Professor W. A. Noyes, University of Illinois.

Secretary: Professor James Kendall, Columbia University.

Member of Council: Professor M. A. Rosanoff, Pittsburgh.

Member of General Committee: Dr. R. F. Bacon, Pittsburgh.

Member of Sectional Committee: Dr. Irving Langmuir, Schenectady.

John Johnston,
Secretary

[1] Vol. 44, p. 885, 1916.

[2] It will, we hope, prove feasible to have all of these papers printed together in some suitable place.